

natural selection by insects, because the spots and streaks of flowers are much less sharply defined.

2. Why is ornamental colouring, as a rule, confined to the male? If the love of beauty is an animal instinct, why, on Darwinian principles, is not beauty developed in the females, the most beautiful females being the most likely to obtain mates and leave offspring? I speak chiefly of birds.

3. Is there any reason to believe that the female has any choice or power of selection whatever? I think that what evidence we have goes to prove that she is passive: and certainly this opinion is supported by the very general fact of the males fighting for the possession of the females.

If the love of beauty is an animal instinct, then Darwinian principles would require that the struggles of the males for the possession of the most beautiful females should develop beauty in the females by natural selection. But we see that the contrary is what takes place—beauty is developed in the male, the fighting sex.

Were a Darwin among birds to watch the ways of the human race, he would probably feel certain that the love of dress and ornament among women is altogether due to a desire to become attractive to men; and he would think those naturalists unsatisfactory, and perhaps mystical, who guessed the truth, that the love of ornament is a natural and healthy human instinct, not confined to either sex or to any age, but stronger in youth than in age, and stronger in woman than in man.

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Psychology of Cruelty

THERE is a passage in Mill's recently published essay on "Nature" which well merits the attention of evolutionary psychologists. It is as follows:—"Again, there are persons who are cruel by character, or, as the phrase is, naturally cruel; who have a real pleasure in inflicting, or seeing the infliction of pain. This kind of cruelty is not mere hardheartedness, or absence of pity or remorse; it is a positive thing; a particular kind of voluptuous excitement. The East, and Southern Europe, have afforded, and probably still afford, abundant examples of this odious propensity." (Page 57.)

Now, I think that this "hateful propensity" is of more common occurrence than even Mr. Mill here gives it credit for. Indeed, I doubt whether anyone is entirely devoid of it, although, of course, everyone who is sufficiently advanced in moral culture to admit of the subordination of the baser instincts to the higher, has been more or less successful in "starving it by disuse." I believe, in short, that this propensity must be regarded as one of the primary instincts of our nature, although, like other instincts, it varies in its original intensity in different individuals, and is further differentially modified by the various influences of education. The nature of this instinct is well expressed by Mr. Mill in the above-quoted phrase, "a particular kind of voluptuous excitement." This, I think, supplies the reason why it is, as a rule, of stronger development in men than in women, and why, as Mill observes, it is of most frequent manifestation in warm climates. It is also worth observing, that although thus akin to the amatory passion, it is of much earlier growth in the life-history of the individual. Indeed, childhood and youth are, in civilised society at least, the seasons when its presence is most conspicuous; in consequence, I suppose, of the restraining power which reflection subsequently brings to bear upon it not as yet having been called into action.

To explain the origin of this instinct by the evolutionary psychology is, I believe, impossible in the present state of our knowledge; for there is no period in the history of the race at which it is conceivable that the latter should have derived any benefit from the birth and development of this peculiar passion. Yet I believe it is now in some persons, were it permitted to assert itself, of even more intensity than is the highly beneficial inclination to which, as we have just seen, it is so strangely allied. To refer to the striking similarity of this passion in man to that which is manifested by monkeys, is not of course to explain its origin; but I am quite sure it is in the monkeys that this explanation is to be sought. Everyone knows that these animals show the keenest delight in torturing others simply for torturing sake, but everyone does not know how much trouble an average monkey will put himself to in order that he may gratify this taste. One example will suffice. A naturalist who had lived a long time in India told me that he has not unfrequently seen monkeys

feigning death for an hour or two at a time, for the express purpose of inducing crows, and other carnivorous birds, to approach within grasping distance; and when one of the latter were caught, the delighted monkey put it to all kinds of agonies, of which plucking alive seemed to be the favourite.

As I am not aware that any other animal exhibits this instinct of inflicting pain for its own sake—the case of the cat with a mouse belonging, I think, to another category—I believe, if its origin is ever to receive a scientific explanation, this will be found in something connected with monkey-life.

PHYSICUS

Migration of Birds

YESTERDAY and to-day (17th and 18th inst.) continuous flights of migrant birds, chiefly fieldfares and redwings, have passed over this place in one uniform direction, from east to west, turning inland to the north-west, as though unwilling to cross Poole Harbour. The procession, so far as it attracted my own notice, began with daybreak of the 17th, and was so rapid and continuous all that day that enormous numbers altogether must have passed over us. Close flocks would come, and then a continuous flight of stragglers, but all in one and the same direction, and with little deviation from a well-defined aerial pathway, as though keeping some visible high-road. Yesterday the flight was down the wind; this morning against it; and although the flight was low and the birds seemed tired, none alighted in this neighbourhood. Whence did they come, and whither are they bound—east or west of this place? Can any of your readers say?

H. C.

Bournemouth, Dec. 18

The Potato Disease

In his letter of last week, Prof. Dyer states that his main object in his previous letter was "to claim for a distinguished English botanist credit for work done by him thirty years ago." In his previous letter this work is defined by Prof. Dyer to be the discovery by the Rev. M. J. Berkeley of the fact that the potato disease was due to the attacks of a parasitic fungus. As the service, with which botanists are familiar, that Mr. Berkeley has rendered in this matter, is the publication and advocacy in this country of the discovery previously made by Montague and others, with a few additional observations of his own, Prof. Dyer would confer a favour on his fellow-botanists by giving a more exact reference to the records which he is so anxious should be duly recognised.

INQUIRER

HELMHOLTZ ON THE USE AND ABUSE OF THE DEDUCTIVE METHOD IN PHYSICAL SCIENCE*

SINCE the translation of the first part of this volume was published, its whole scientific tendency, and specially a series of individual passages in it, have been subjected to a more than vigorous criticism by Mr. J. C. F. Zöllner in his book "On the Nature of Comets." I do not think it necessary to answer expressions of feeling in reference to personal characteristics of the English authors or of myself. I have as a rule considered it necessary to reply to criticisms of scientific propositions and principles only when new facts were to be brought forward or misunderstandings to be cleared up, in the expectation that, when all data have been given, those familiar with the science will ultimately see how to form a judgment even without the discursive pleadings and sophistical arts of the contending parties. If the present treatise were intended only for fully educated men of science, Zöllner's attack might have been left unanswered. It is, however, essentially designed for students also, and as junior readers might perhaps be misled by the extreme assurance and the tone of moral indignation in which our critic thinks himself justified in expressing his opinions, I consider that it would be useful to answer the attacks made on the two English authors, so far as may

* Translated by Prof. Crum Brown from Helmholtz's preface to the second part of the German edition of Thomson and Tait's "Natural Philosophy," vol. i.

be necessary to enable the reader to make out the truth by considering the matter for himself.

Among the scientific investigators who have especially directed their efforts towards the purification of physical science from all metaphysical infection and from all arbitrary hypotheses, and, on the contrary, have striven to make it more and more a simple and faithful expression of the laws of the facts, Sir W. Thomson occupies one of the first places, and he has consciously made precisely this his aim from the beginning of his scientific career. This very thing seems to me to be one of the chief services rendered by the present book, while in Mr. Zöllner's eyes it forms its fundamental defect. The latter would like to see, instead of the "inductive" method of the scientific investigator, a predominantly "deductive" method introduced. We have all hitherto employed the inductive process to discover new laws, or, as the case may be, hypotheses; the deductive to develop their consequences for the purpose of their verification. I do not find in Mr. Zöllner's book a distinct declaration by which his new mode of procedure may be distinguished from that generally followed. Judging from what he aims at as his ultimate object, it comes to the same thing as Schopenhauer's Metaphysics. The stars are to love and hate one another, feel pleasure and displeasure, and to try to move in a way corresponding to these feelings. Indeed, in blurred imitation of the principle of Least Action (pp. 326, 327), Schopenhauer's Pessimism, which declares this world to be indeed the best of possible worlds, but worse than none at all, is formulated as an ostensibly generally applicable principle of the smallest amount of discomfort, and this is proclaimed as the highest law of the world, living as well as lifeless.

Now, that a man who mentally treads such paths should recognise in the method of Thomson and Tait's book the exact opposite of the right way, or of that which he himself considers such, is natural; that he should seek the ground of the contradiction, not where it is really to be found, but in all conceivable personal weaknesses of his opponents, is quite in keeping with the intolerant manner in which the adherents of metaphysical articles of faith are wont to treat their opponents, in order to conceal from themselves and from the world the weakness of their own position. Mr. Zöllner is convinced "that the majority of the present representatives of the exact sciences are wanting in a clearly conceived intelligence of the first principles of the theory of perception" (p. viii.) This he tries to confirm by reference to supposed gross errors made by several of them.

Here then, of course, Messrs. Thomson and Tait must submit to the ordeal. They have, in paragraphs 381-385 of the present book given expression to their conviction as to the right use of scientific hypotheses. They, in paragraph 385, find fault with hypotheses which are too remote from observable facts, and select, as instances of their injurious influence, naturally only such as, by their extensive diffusion and by the authority of their originators, have been really influential. In this connection they place side by side the law of electrical action at a distance propounded by our countryman, W. Weber, and the emission theory of light as worked out by Newton. This juxtaposition is the best proof that the English authors had nothing in view that should wound a healthy German national feeling.

It has not as yet, I believe, come to such a pass in Germany—it is to be hoped it never will—that hypotheses may not be criticised, whatever be the eminence of their propounders. Should it actually ever come to this, then indeed Mr. Zöllner and his metaphysical friends would be justified in bewailing, or it may be in triumphing over, the destruction of German science. No one can be blamed for having advanced a hypothesis which the further progress of science shows to be inadmissible, just as it is no discredit for one who has to seek his way in

an entirely unknown country to take the wrong road for once, in spite of his utmost attention and consideration. It is further obvious that whoever regards as erroneous a hypothesis which has captivated the minds of a large number of scientific men must necessarily hold that it, for the time being, injures and retards the progress of science, and will be justified in expressing this opinion, if it becomes his duty to advise, according to his matured conviction, a student as to the path he should follow.

One of the arguments which Sir W. Thomson has adduced to prove the inadmissibility of Weber's hypothesis, is that it contradicts the law of the conservation of energy. I was also obliged to bring forward the same allegation somewhat later in a paper* published in the year 1870. Now Mr. Zöllner, relying on the authority of Mr. C. Neumann, has assumed that this allegation is erroneous. On the contrary, Weber's law seems to him to be another universal law of all forces in nature (it is not explained how these different universal laws agree with one another), and he devotes twenty pages of his introduction to the purpose of airing his indignation at the intellectual and moral dulness of those who attack it. Mr. Zöllner will, no doubt, since then, have become aware that it is at least imprudent, without other support than the authority of one of the parties in a scientific debate, to try to help the other by libellous remarks, apart from the consideration that by such means one can contribute nothing to the settlement of the dispute, but perhaps much to its embitterment. Mr. C. Neumann was himself a party in this affair; my objections applied also to the theory of electrodynamic actions, to which he then adhered. He has since then given up this theory. He and also Mr. W. Weber thought that they could maintain the original theory of the latter, if they took into consideration the co-operative action of molecular forces in the case of closely approximated electrical masses. I then, in my second contribution to the theory of electrodynamics,† pointed out that the assumption of molecular forces does not stop the leak in Weber's theory. In the meantime Mr. C. Neumann himself, before he knew of my second paper, had given up the attempt to found a theory of electrodynamics upon Weber's law, and had tried to devise a new law for that purpose.

And here, in reference to the emphatic way in which our opponent speaks of the deductive method, I would make the following remarks on this example:—According to the view hitherto held by the best scientific investigators, the deductive method was not only justified, but indeed required, when the admissibility of a hypothesis was to be tested. Every legitimate hypothesis is an attempt to establish a new and more general law which shall include under it more facts than those hitherto observed. The testing of it consists in this, that we seek to develop *all* the consequences which flow from it, in particular those which can be compared with observable facts. I should therefore imagine the first duty of those who would support Weber's hypothesis to be, among other things, to see whether this hypothesis can explain the most general fact, that electricity, when no electromotive forces act on it, remains at rest in all electrical conductors, and is therefore capable of continuing in stable equilibrium. If Weber's hypothesis implies the contrary of this, as I have attempted to prove, then the next thing to be done would be to look out for such a modification of it as would render stable equilibrium possible in the largest as well as in the smallest conductors. According to my view, this would have been a right course, and the one required by the deductive method, but not to call a halt when inconvenient consequences appear, and excuse oneself with the plea that the right differential equations for the motion of

* "Ueber die Bewegungsgleichungen der Elektricität für ruhende leitende Körper." Borchart, *Journal für Mathematik*, Bd. 72, 75.

† Borchart, *Journal für Mathematik*, Bd. 75.

electricity in accordance with Weber's law had not yet been discovered. And if some one else takes this trouble, then he who considers himself a representative καὶ ἐξηγής of the deductive method should applaud him, instead of charging him with impiety, even if the results of the inquiry should turn out to be inconvenient for the Icarus flight of speculation.

As Mr. Zöllner does not put himself forward as a mathematician—on the contrary, informs us on pages 426 and 427 of his book that the too frequent use of mathematics cramps the conscious activity of the understanding and is a convenient means of satisfying vanity ; and besides, in many passages, constantly repeats his expression of contempt for those who think they can refute his speculations by pointing out mistakes in differentiation and integration—we ought not to judge him too severely in the matter of Weber's law. No doubt it is scarcely reasonable for one who thinks himself entitled to be shaky in his mathematics, to take upon himself to pronounce upon matters which can be decided by mathematical investigation only. His "Theory of Comets," which may surely be regarded as in his opinion a model specimen of how the right methods are to be employed, gives, besides this, other much more popular examples of the same peculiar way of using or not using deduction, examples the consideration of which may be reserved for another more suitable opportunity.

(To be continued.)

MOVEMENTS OF THE HERRING

THE mysterious disappearance of the body of herring which used to frequent Loch Fyne has directed renewed attention to the natural history of that fish. This is now the second time that the shoal of herrings which made Loch Fyne its *habitat* has deserted that celebrated sheet of water. No scientific opinion has yet been given as to the cause of this disappearance. A number of fishermen, resident on the Loch, say the herrings have been frightened away in consequence of persons fishing for them with a trawl net—which is, of course, nonsense ; but not more nonsensical than the reasons assigned for the desertion by herring of other localities. As the so-called trawl-fishing of Loch Fyne (the net used is in reality a seine) was not in existence when the fish forsook the Loch on a former occasion, and were absent for a period of six years, the opinion of these men may be passed over as unworthy of serious consideration. Writers in the local newspapers, while inclined to favour the opinions of the drift-net men, that is, those who assert that the trawl-fishers have scared away the fish, also ask whether the spawning-beds may not have been in some way interfered with, and whether the body of fish frequenting the Loch may not from some unknown cause have departed before depositing their seed. If so, in what year would that occur ? In other words, how long is it before the herring spawn of any given year comes to life, and at what period will the fish then born become reproductive ?

These are events in the natural history of the herring, the dates of which have not yet been authoritatively settled. They are points, indeed, which have not yet been decided as regards any of our fish, except, *perhaps*, the salmon (*Salmo salar*), which has been nursed into life under a system that may be called artificial, that admitted of the young fish being watched, and their growth traced stage by stage, by means of certain signs and marks. It is thought that we may speak of the natural history of the salmon with more confidence than that of any of our other food-fishes. It is unfortunate that their studies of the natural history of the herring have not yet enabled naturalists to determine with exactitude how long it takes that fish to come to maturity.

Most varied opinions have been given on these points of herring life. Some persons have even gone the length of asserting that *Clupea harengus* and its congener *Clupea pilchardus* are able to perpetuate their kind within a year of their birth ; even at the age of ten months ! It has also been asserted that a herring is able to breed twice a year. Other opinions have been given, which assign to the herring a much longer period of growth, namely, that it requires from three to five years to reach maturity. Yarrell, again, and also Mitchell, think that it becomes reproductive in so short a period as eighteen months. What we may hold that we really do know is, that the eggs of the herring can be hatched within twenty days after their contact with the milt of the male fish. This has been proved by visiting the spawning places of the animals. On one visit all was spawn, everything that came in contact with the spawning-beds being covered with the seed of the herring ; at the next visit, a fortnight after, the spawn was all gone ; it had become vivified—and in proof of the fact, young herrings could in two or three weeks after be found in shallow places varying from an inch to two and even three inches in length. The probable time between the spawning of the fish and the fry reaching the dimensions named would be about forty days. How fast the young ones grow after that has not been authoritatively ascertained. It is thought, however, that if young herring reach the size of, say two-and-a-half inches, in forty days, it is not unreasonable to expect them to continue growing at the same ratio.

In the case of *Salmo salar*, the period necessary for the incubation of the egg has been determined beyond dispute. It ranges from 90 to 130 days. The growth of the young fish, after a time, if those who have watched it have not been deceived, is very rapid. At first, however, the salmon grows very slowly. A salmon hatched in March last may still be a very tiny animal, even after it is twelve months and in some cases two years old. In a year, however, it may be four or five inches long, and ready to migrate to the sea. There is a curious feature in the natural history of the salmon, the law of which has never yet been discovered—it is a riddle, in fact, even to the most scientific observers : only one half of the salmon of any particular hatching develop into what is called the *smolt*, or migratory stage, at the end of about twelve or fifteen months from the time of their being hatched. The other moiety of the brood does not seek the sea or take on the migratory dress till the expiry of a little over two years from the time of birth ! One half of the fish, therefore, will at one and the same time be tiny creatures, about three inches long, whilst the other moiety will be five inches in length, and of corresponding girth ; but these dimensions, it must be confessed, show no great rapidity of growth. Indeed, it is not till after the salmon proceeds to the sea that its growth becomes at all rapid ; but, notwithstanding this rapidity, it must, we think, be a considerable number of years before a salmon can attain to the weight of fifty or sixty pounds ; although the smolt, it is affirmed by those who have watched it, returns as a grilse to its native waters in about three months, its size and weight being very largely increased.

The herring, as we all know, is a fish that never attains to any great size, and the weight of which may be counted in ounces. The question to be answered is this : Do small fish grow to maturity quicker than large ones ? It has been asserted, in some quarters, that the herring grows quite as rapidly as the smolt does *after* it reaches the salt water, and the rate of growth there appears magical, when contrasted with its slow progress during the first year of its existence, or it may be, as has been already explained, the first two years. We are not, however, without a certain kind of proof of the rate at which the herring grows, which is better than reasoning analogically. It is quite fair to conclude that if herrings attain a size of about three inches within forty days or so of their birth,